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Ross Patent Law Office P.O. Box 2138 Del Mar, CA 92014			GREENE, DANIEL LAWSON	
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			3663	

DATE MAILED: 04/06/2006

Please find below and/or attached an Office communication concerning this application or proceeding.

Office Action Summary	Application No.	Applicant(s)	
	10/647,774	STORMS, EDMUND K.	
	Examiner	Art Unit	
	Daniel L. Greene Jr.	3663	

-- The MAILING DATE of this communication appears on the cover sheet with the correspondence address --
Period for Reply

A SHORTENED STATUTORY PERIOD FOR REPLY IS SET TO EXPIRE 3 MONTH(S) OR THIRTY (30) DAYS, WHICHEVER IS LONGER, FROM THE MAILING DATE OF THIS COMMUNICATION.

- Extensions of time may be available under the provisions of 37 CFR 1.136(a). In no event, however, may a reply be timely filed after SIX (6) MONTHS from the mailing date of this communication.
- If NO period for reply is specified above, the maximum statutory period will apply and will expire SIX (6) MONTHS from the mailing date of this communication.
- Failure to reply within the set or extended period for reply will, by statute, cause the application to become ABANDONED (35 U.S.C. § 133). Any reply received by the Office later than three months after the mailing date of this communication, even if timely filed, may reduce any earned patent term adjustment. See 37 CFR 1.704(b).

Status

1) Responsive to communication(s) filed on 25 January 2006.
 2a) This action is FINAL. 2b) This action is non-final.
 3) Since this application is in condition for allowance except for formal matters, prosecution as to the merits is closed in accordance with the practice under *Ex parte Quayle*, 1935 C.D. 11, 453 O.G. 213.

Disposition of Claims

4) Claim(s) 1-17 is/are pending in the application.
 4a) Of the above claim(s) 1-9, 11, 12 and 14-16 is/are withdrawn from consideration.
 5) Claim(s) _____ is/are allowed.
 6) Claim(s) 10, 13 and 17 is/are rejected.
 7) Claim(s) _____ is/are objected to.
 8) Claim(s) _____ are subject to restriction and/or election requirement.

Application Papers

9) The specification is objected to by the Examiner.
 10) The drawing(s) filed on _____ is/are: a) accepted or b) objected to by the Examiner.
 Applicant may not request that any objection to the drawing(s) be held in abeyance. See 37 CFR 1.85(a).
 Replacement drawing sheet(s) including the correction is required if the drawing(s) is objected to. See 37 CFR 1.121(d).
 11) The oath or declaration is objected to by the Examiner. Note the attached Office Action or form PTO-152.

Priority under 35 U.S.C. § 119

12) Acknowledgment is made of a claim for foreign priority under 35 U.S.C. § 119(a)-(d) or (f).
 a) All b) Some * c) None of:
 1. Certified copies of the priority documents have been received.
 2. Certified copies of the priority documents have been received in Application No. _____.
 3. Copies of the certified copies of the priority documents have been received in this National Stage application from the International Bureau (PCT Rule 17.2(a)).

* See the attached detailed Office action for a list of the certified copies not received.

Attachment(s)

1) <input checked="" type="checkbox"/> Notice of References Cited (PTO-892)	4) <input type="checkbox"/> Interview Summary (PTO-413)
2) <input type="checkbox"/> Notice of Draftsperson's Patent Drawing Review (PTO-948)	Paper No(s)/Mail Date. _____ .
3) <input type="checkbox"/> Information Disclosure Statement(s) (PTO-1449 or PTO/SB/08) Paper No(s)/Mail Date _____ .	5) <input type="checkbox"/> Notice of Informal Patent Application (PTO-152)
	6) <input type="checkbox"/> Other: _____ .

DETAILED ACTION

Election/Restrictions

1. Applicant's election without traverse of Group II species 5A, 6C (palladium and gold) and 7B in the reply filed on 1/25/2006 is acknowledged.
2. Claims 1-9, 11, 12, and 14-16 are withdrawn from further consideration pursuant to 37 CFR 1.142(b) as being drawn to a nonelected Invention/species, there being no allowable generic or linking claim. Election was made **without** traverse in said reply filed on 1/25/2006.

The following is a quotation of the first paragraph of 35 U.S.C. 112:

The specification shall contain a written description of the invention, and of the manner and process of making and using it, in such full, clear, concise, and exact terms as to enable any person skilled in the art to which it pertains, or with which it is most nearly connected, to make and use the same and shall set forth the best mode contemplated by the inventor of carrying out his invention.

The following is a quotation of the second paragraph of 35 U.S.C. 112:

The specification shall conclude with one or more claims particularly pointing out and distinctly claiming the subject matter which the applicant regards as his invention.

Specification

3. **The specification is objected to under 35 U.S.C. § 112, first paragraph, as failing to provide an adequate written description of the invention and as failing to adequately teach how to make and/or use the invention i.e. failing to provide an enabling disclosure.**

There is no reputable evidence of record to support any allegations or claims that the invention involves nuclear fusion, nuclear reactions, Low Energy Nuclear Reactions (LENR) or Chemically Assisted Nuclear Reactions (CANR) nor, that any allegations or

claims of "excess heat", are valid and reproducible, nor that the invention as disclosed is capable of operating as indicated, by depositing palladium and gold onto a platinum cathode and applying pulsed electric power between said cathode and an anode in an electrolytic environment, and capable of providing a useful output.

To the contrary, there is documentary evidence showing that the instant applicant's allegations of excess heat cannot be relied on as valid. See for example, Shanahan (VIII), the Dec. 1, 2004 final report of the DOE panel on cold fusion, as well as the 18 Reviewers comments, the November 1989 Report of the ERAB to the DOE, Shanahan (V), Shanahan (VI), Shanahan (VII) and Miles et al (II) (NAWCWPNS TP 8302).

The 2004 DOE Reviewers were presented with applicant's theoretical framework of the instant application, however they did not find applicant's theories convincing, e.g. note the comments from pages 3 and 4 of the DOE Dec. 1, 2004 Report:

Regarding applicant's field of endeavor in general, the Dec. 1, 2004 DOE Report on page 3 states:

"The reviewers who did not find the production of excess power convincing cite a number of issues including: excess power in the short term is not the same as net energy production over the entire of time of an experiment; all possible chemical and solid state causes of excess heat have not been investigated and eliminated as an explanation; and production of power over a period of time is a few percent of the external power applied and hence calibration and systematic effects could account for the purposed net effect. Most reviewers, including those who accepted the evidence and those who did not, stated that the effects are not repeatable, the magnitude of the effect has not increased in over a decade of work, and that many of the reported experiments were not well documented." (Underlining added).

and, regarding applicant's instant theories, page 4:

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"Reviewers expert in nuclear physics noted that the cold fusion mechanism put forward by proponents is not in accord with presently accepted knowledge of D+D fusion. Specifically, D+D fusion is accompanied by the production of protons, neutrons, tritons, ^3He , ^4He and high energy gamma rays, all in well known proportions. The fusion channel resulting in ^4He and high energy gamma rays occurs approximately only once for every 10^7 D+D fusion reactions. These characteristic proportions for the production of the fusion products are found for every energy of the incident deuteron measured so far, down to the lowest that has been measured.

The review document and oral presentations made the argument that the branching ratios are different at low energies and that in cold fusion, ^4He fusion channel is predominant. According to the review document, no high energy gamma rays appear to accompany the ^4He , as is observed in D-D fusion reactions. Instead, the approximately 24 MeV in energy resulting from D-D fusion was purported to appear as heat in the material lattice. To explain these unusual characteristics the reviewers were presented with a theoretical framework that purported to describe how collective energy from the material lattice couples to a deuteron pair to induce fusion, how the only fusion reaction channel that occurs would be the production of ^4He , and how all the energy is coupled back into the material in the form of heat instead of high energy gamma-rays. The reviewers raised serious concerns regarding the assumptions postulated in the proposed theoretical model for the explanation for ^4He production. (Underlining added)

Note in addition to the above quoted portion from page 4 of Dec. 1, 2004 DOE Report, pages 9, 25, 26, 34, 40 of the Dec. 1, 2004 "Reviewer Comments".

Note particularly said page 9, which states:

It seems to me that the authors should have had a section, which carefully addressed the many cogent arguments offered in the literature tending to discredit the existence of CF. For example, K. Shanahan (Thermochim. Acta, 387 (12002) 95) has argued that the excess heats (E. Storms, ICCF8, (2001) p. 55-61) can be explained on the basis of fluctuations in the calibration. He has elsewhere pointed out that the catalytic recombination of D₂ and O₂ can also be a factor for the excess heat since the latter is evolved at the anode and the former will be evolved at the cathode once a steady state is reached. (Underlining added)

and said page 25 to said page 26 states:

"Compared with the experimental efforts, the theoretical work is even more unconvincing. To make a case for nuclear reactions to happen in condensed matter at low energies as suggested or speculated by experiment, theory has to be formulated to explain (1) the enhanced nuclear reaction rate in the condensed matter environment, (2) the completely different Branching Ratio for the d-d reaction from the gas phase, and (3) the mechanism for the dissipation of the 24 MeV energy through the lattice. None of these has been demonstrated, nor any promising directions have been shown. Because of these deficiencies, one is having a difficult time in understanding the experimental implications. My comments on each of the three areas are given below.

- (1) It was mentioned at several places in the documents or presentations that high deuterium loading might result in double occupation of the octahedral site in Pd, and thus bring the deuterium atoms closer together and enhance their interaction. However, it has been shown by first-principles electronic calculations [P. K. Lam and R. Yu, Phys. Rev. Lett. 63, 1895 (1989)] that the lowest energy configuration for two deuterium atoms at one octahedral site is an arrangement along the (111) orientation with a D-D distance of 1.3 Angstroms, which is still significantly larger than that in the molecule (0.74 Angstroms). On the other hand, so far the quantum nature of deuterium in the metal has not been taken into account. Previous electronic calculations did show that the potential well was not harmonic, and the zero-point motion was quite significant [C. Elsasser et al., J. Phys.: Condensed Matter 4, 5207 (1992)]. Most importantly, the two deuterium atoms have to be described by correlated wave functions with a mutual interaction $V(r_1, r_2) \neq V(r_1-r_2)$ inside the crystal, which is completely different from the situation in usual scattering experiments on the gas phase. These critical issues for a decent theory have been completely ignored so far and would require an interdisciplinary effort in the future.
- (2) The most puzzling part for nuclear theory is the lack of neutrons commensurate with the heat production and the complete reversal of the ratio for the reaction channels. This is still the crucial and seemingly insurmountable physics problem that needs to be resolved.
- (3) The lack of gamma rays being detected from the sample forced researchers to invent a coupling between the nuclear interaction and lattice vibrations. Being able to write down the equations does not imply physical justifications. An effective interaction normally involves some type of fundamental interactions that lead to the coupling. For example, the effective electron-electron interaction mediated by phonons, through electron-phonon coupling, leads to superconductivity. Under the carpet, the electron-phonon coupling arises from the electromagnetic interaction, one of the four known fundamental interactions in physics. To create a

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coupling between nuclear interaction and phonons at such a low energy region (namely, the electromagnetic interaction) is beyond one's imagination at the moment.

A series of conjectures is formulated in Hagelstein's paper, but a lot of them appear to be too ad hoc. In particular, the phonon mediated site-other-site reaction is, at most, a "conjecture". The exchange of a large angular momentum with phonons is unprecedented. This paper has a lot of holes and is not likely to go through any peer review process of reputable journals. Better theory could be done, however, by considering the points mentioned in (1).

In summary, in my opinion, there is no theory for low-energy nuclear reaction yet. Therefore, the burden of proof lies on experiment. Although there is still a long way to go, the experimental efforts are moving in the right direction to provide a converging conclusion, one way or the other. The current evidence is not sufficiently conclusive to demonstrate that nuclear reactions occur in metal deuterides yet."

and said page 34 states:

"If excess heat originates from nuclear fusion than I agree with several of the presenters in that the rate of fusion must be 10^{11} per second. If one has a burst as is claimed then the rate would have to reach 10^{13} or so. Assuming the excess heat production of 60 hours as claimed in the Hagelstein review means that one will have about 10^{14} or 10^{15} tritons in the cell. The same would be said for He. It would be very easy to detect this level of tritium and the fact that they don't means that one has to hypothesize that this highly loaded rod into which as much deuterium has been placed as possible will absorb the produced tritium. Again, we are looking at a low level effect that we can never disprove. Perhaps the hardest effect to accept is that of having the d+d [alpha] with no other output than heat. We are asked to accept the fact that 23 MeV of kinetic energy of the alpha particle can be transferred to a lattice with no effect other than heat. There is a large body of literature from the reactor field dealing with the impact that recoiling nuclei have on fuel rods and the damage they produce. There is no reason to suspect that only in Pd or Ti will there be a different coupling mode so that instead of the kinetic energy of the [alpha] particle producing recoil into the metal lattice they "gently" transfer energy to heat through coupling to a huge number of phonons. The nuclear energy scale is so much larger than that of the phonons, about 10^7 or more, that recoil damage to nuclear reactor fuel rods leads to their embrittlement and if left in too long, crumbling."

and, said page 39 to page 40 states:

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"Finally, I make several comments on the presented theoretical speculations. First and foremost, it must be emphasized that this field is not theory driven. Conventional nuclear and atomic theory predicts that no d+d fusion reactions can occur at room temperature at a measurable rate, even in the presence of a metal catalyst. If the experimental results of significant energy release in electrolytic cells were correct and the energy release were due to nuclear fusion, the theory would need to explain not one, but two "miracles": (1) why the fusion rate is enhanced by tens of orders of magnitude, and (2) why the branching ratios for the final states are modified by orders of magnitude. In vacuum, the $^3\text{H}+\text{p}$ and $^3\text{He}+\text{n}$ channels are 10 million times more likely than the $^4\text{He}+\square$ channel. No evidence is found for the dominance of either of these final states (the radiation hazard from neutrons and gamma rays would be quite severe at the level of the fusion reactions required to explain the observed intermittent power output).

The presented theoretical arguments for a qualitative change of nuclear the reaction mechanisms by solid state environment are not credible. The situation is not comparable to that of the Mossbauer effect, because:

- The lattice vibrations would need to couple to the internal nuclear degrees of freedom, not to the motion of the whole nucleus as in the Mossbauer effect. For the required energy transfer (>20 MeV) this coupling would be dominated by the giant dipole resonance of the Pd nucleus, which would cause a large reduction in the coupling to the lattice vibrations.
- The energy transfer for the d+d \rightarrow ^4He reaction of more than 20 MeV lies far outside the energy scale of the phonon spectrum. As Hagelstein emphasizes, this would imply that the nuclear system has to transfer its energy to a very highly occupied phonon mode (billions of phonons coherently excited). Although such a mechanism might conceivably work energetically, it would be associated with a matrix element of astronomical weakness. For an interaction of multipolarity L, the matrix element would be suppressed by a factor $(kR)^{2L}$ for each phonon, where k is the phonon wave number and R denotes the nuclear radius. Since $k\square 1/a$, where a is the lattice constant, any interaction of this type would be unimaginably weak even if the interaction were of dipole character, and the coupling would be even weaker for higher multipoles.

I am convinced that simple order-of-magnitude estimates of this kind could quickly rule out any of the exotic mechanisms proposed by Hagelstein. The problem is not with the formalism he proposes to apply –R-matrix theory, for example, is a standard and proven staple of nuclear reaction theory– but I cannot see, and he does not demonstrate, how any of the proposals could result in required large reaction rates. And no matter how the energy is transferred from the excited compound nucleus to the lattice, the two deuterium nuclei have to

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fuse first requiring them to overcome the repulsive Coulomb barrier, which impedes fusion at room temperature beyond observability." (Underlining added)

As to further issues with the instant alleged invention, see page 22 of the Dec. 1, 2004 "Reviewer Comments", which states:

"There is also the problem of reports of excess heat from systems very different than the Pd/D₂O system, apparently from groups that have had experience with calorimetry. For example, Storms, who wrote a good discussion of calorimeters and their possible errors (ref. 3), reported excess heat effects with the Pt/D₂O system (ref. 82). Note that in other reports systems with Pt cathodes are considered controls or blanks in calorimetric measurements. In this work, excess power bursts with a Pt cathode, analogous to those reported with the Pd/D₂O system, were seen. Storms also reports excess power from Ag/D₂O². Similarly, there have been reports of excess energy by several groups in a light water system: Ni/H₂O, Na₂CO₃³. While these results do not negate the careful work on the Pd/D₂O system, they are disconcerting. Either they are correct, which now brings to question the nuclear mechanism proposed that is largely based on deuterium in a Pd lattice, or they are incorrect, which then exposes serious problems with calorimetric measurements of the type reported.

In a general summary of the calorimetric results, the observation of sudden and prolonged temperature excursions (bursts of excess heat), has been made a sufficient number of times that, even if not totally reproducible, still have not been explained in terms of conventional chemistry or electrochemistry (a conclusion also made in the 1989 ERAB report). However the systems are sufficiently complicated, the measurement sufficiently difficult, and the effects sufficiently small, that it is difficult to conclude from these effects alone that nuclear processes are involved. Even with all of the careful work that has been done on electrochemical cells and calorimetry, the system is still not under experimental control, in the sense that one knows exactly the materials needed and the operating conditions to get the same results, even semiquantitatively, every time." (Underlining added)

and page 28 which states:

"The production of excess energy through low energy nuclear processes is the central issue in cold fusion. I am not persuaded that such energy has been produced. Although there have been many experiments reporting excess energy since the 1989 ERAB report, all of these that I have read about suffer in varying degrees from two serious shortcomings that were pointed out in the ERAB report.

- 1) Calorimetry

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Excess energy is never more than a small fraction of the energy delivered to the system, typically a few percent, so that a small error in calorimetry can yield a large error in an estimate of excess energy. Unfortunately, experimental results are almost always presented as data on excess power, often starting hundreds of hours after the experiment starts, but not on excess energy. No direct inference about energy can be made from such data-what is required is a complete inventory of energy flow into and out of the apparatus from the moment the cell is turned on until the moment the experiment is terminated. Normal experimental practice requires that the accuracy of such an inventory be determined experimentally by dummy runs, and the experimental scatter is consistent with the known sources of uncertainty.

2) Reproducibility

The lack of reproducibility continues to be a serious problem. None of the important phenomena can be duplicated reliably. This has made it impossible to obtain a quantitative understanding of what is taking place.”

and page 29 which states:

“I find that the overall situation has not fundamentally changed since 1989 when the ERAB report was written: the experiments are poorly executed, the phenomena are not reproducible, and the claims of “new physics” are not plausible. Consequently, my recommendations are similar to the major recommendations of that report.”

and page 30 which states:

“This willing substitution of power for energy seems endemic to the field. However, for the following reasons, I do not regard reports of excess energy as credible unless the accuracy of the calorimeter for measuring energy, as contrasted to power, is understood and well confirmed by actual experiments:

- 1) Because excess heat is typically only small fractions of the total energy to a cell during a run, small errors in power measurements can become large errors in excess heat measurements.
- 2) Operating conditions can vary during an experiment, particularly if energy is released rapidly, which makes it difficult to determine the long-term energy balance from power calibrations.
- 3) Consequently, the only way to reliably determine the accuracy of an experiment is to calibrate the electrolytic cell for total energy-in vs. total energy-out over the entire duration of a run, with a heat profile similar to an

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experimental run, and to check this calibration a number of times. If the variations in the measurements of energy-in and energy-out for these calibration runs are consistent with the calculated uncertainty based on an analysis of the known sources of error, then one finally has a credible estimate of the accuracy of the method. I have not seen the results of such a procedure."

and, page 31 which states:

"The presentations and written material presented such a confused picture that it is almost impossible to tie things together. There are conflicting claims amongst the advocates, inconsistencies amongst seemingly similar experiments and a general feeling amongst the proponents that the "system" is keeping them from publishing their results and getting DOE funding."

Note the statements on page 47 of the 1989 ERAB Report concerning lack of estimates of precision and the similarities with applicant's instant specification pages 80-82 and Table 1:

"A surprising aspect of the calorimetry related to cold fusion is the lack of attention that has been given so generally in reports of excess heat to the statistical assessment of errors. It is evident on the face of the data in some reports that a group's claim of excess heat is not supported with results of sufficient precision to allow such a conclusion. More usually, it is not possible to assess precision from reported results, because the result is reported from a single run and no error bars are provided for the measured parameters.

Conclusions in this arena simply cannot be accepted without a thorough assessment of the measurement errors. In its visits and conversations, the members of the Panel were struck repeatedly by the absence of critical assessments of this kind. Several different kinds of problems were common:..." (underlining added)

Note also, the conclusion of the 1989 ERAB or DOE Report on pages 36+ which state:

- (1) Based on the examination of published reports, reprints, numerous communications to the Panel and several site visits, the Panel concludes that the experimental results of excess heat from calorimetric cells reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion.

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- (2) A major fraction of experimenters making calorimetric measurements, either with open or closed cells, using Pd cathodes and D₂O, report neither excess heat nor fusion products. Others, however, report excess heat production and either no fusion products or fusion products at a level well below that implied by reported heat production. Internal inconsistencies and lack of predictability and reproducibility remain serious concerns. In no case is the yield of fusion products commensurate with the claimed excess heat. In cases where tritium is reported, no secondary or primary nuclear particles are observed, ruling out the known D+D reaction as the source of tritium. The Panel concludes that the experiments reported to date do not present convincing evidence to associate the reported anomalous heat with a nuclear process.
- (3) The early claims of fusion products (neutrons) at very low levels near background, from D₂O electrolysis and D₂ gas experiments, have no apparent application to the production of useful energy. If confirmed, these results would be of scientific interest. Recent experiments, some employing more sophisticated counter arrangements and improved backgrounds, found no fusion products and placed upper limits on the fusion probability for these experiments, at levels well below the initial positive results. Based on these many negative results and the marginal statistical significance of reported positive results, the Panel concludes that the present evidence for the discovery of a new nuclear process termed cold fusion is not persuasive.
- (4) Current understanding of the very extensive literature of experimental and theoretical results for hydrogen in solids gives no support for the occurrence of cold fusion in solids. Specifically, no theoretical or experimental evidence suggests the existence of D-D distances shorter than that in the molecule D₂ or the achievement of "confinement" pressure above relatively modest levels. The known behavior of deuterium in solids does not give any support for the supposition that the fusion probability is enhanced by the presence of the palladium, titanium, or other elements.
- (5) Nuclear fusion at room temperature, of the type discussed in this report, would be contrary to all understanding gained of nuclear reactions in the last half century; it would require the invention of an entirely new nuclear process.

What is most noteworthy in the DOE Dec. 1, 2004 Report, is that after reviewing the submissions and oral presentations by the cold fusion community, which was based on 15 years of work in the cold fusion field, the panel came to the same conclusion as

that of the Nov. 1989 DOE Report. Note in this respect, the Conclusion on page 5 that states:

"While significant progress has been made in the sophistication of calorimeters since the review of this subject in 1989, the conclusions reached by the reviewers today are similar to those found in the 1989 review.

The current reviewers identified a number of basic science research areas that could be helpful in resolving some of the controversies in the field, two of which were: 1) material science aspects of deuterated metals using modern characterization techniques, and 2) the study of particles reportedly emitted from deuterated foils using state-of-the-art apparatus and methods. The reviewers believed that this field would benefit from the peer-review processes associated with proposal submission to agencies and paper submission to archival journals." (Underlining added).

Applicants disclosed and claimed invention is considered as being based on the "cold fusion" concept set forth by Fleischmann and Pons (hereinafter, F and P)(see the 3/24/89 article by D. Braaten). This concept relies on the incorporation of deuterium into a metal lattice. While F and P relied on electrolysis of heavy water to incorporate deuterium into the metal lattice, it was also known that as a variation, the deuterium could be incorporated into the metal lattice by bringing the metal into contact with deuterium gas or by deposition of special materials onto the cathode surface.

The present application shows that applicant's invention merely differs from the cold fusion system of F and P by the use of gold as said special material being deposited on the cathode. Thus it is clear that applicant's invention is just a variation of the cold fusion concept set forth by F and P.

However, as set forth more fully below, this "cold fusion" concept is still no more than just an unproven concept or theory.

Subsequent to the announcement of the cold fusion concept by F and P, many laboratories have attempted to confirm the results of F and P.

The results of these attempts at confirmation were primarily negative and even of the few initial positive results, these were generally either retracted or shown to be in error by subsequent experimenters (see for example, the article by Stipp in the Wall Street Journal and the article by Browne in The New York Times (particularly page A22)).

The general consensus by those skilled in the art and working at these various laboratories is that the assertions by F and P were based on experimental errors (e.g. see The New York Times article by Browne, Kreysa et al, Lewis et al, Hilts, Ohashi et al, Miskelly et al, and Chapline).

It was also the general consensus by those skilled in the art and working at these various laboratories that there is no reputable evidence to support the allegation or claim of excess heat production, nor, is there any reputable evidence of neutron, gamma ray, tritium, helium production, etc., to support the allegations or claims that nuclear reactions are taking place. See for example (in addition to the above listed references), Cooke, Alber et al, Faller et al, Cribier et al, Hajdas et al, Shani et al, Ziegler et al, Price et al, Schrieder et al and page A3 of the 3/29/90 edition of The Washington post (which refers to the negative findings of a physicist who had tested Pon's own cold fusion apparatus, for nuclear output (for a more complete analysis of said "negative findings", note the article by Salamon et al)). Also of interest in this respect is the Cooke reference which on pages 4 and 5 refers to the attempts at Harwell

to obtain "cold fusion" and that Fleischmann (of F and P) had requested help from Harwell in verifying the cold fusion claims. Said page 5 also indicates that data was collected in Frascatti-type (i.e. gaseous) experiments.

The last paragraph on said page 5 states:

"After three months of around-the-clock work at a cost of over a half a million dollars, the project was terminated on June 15. This program is believed to be one of the most comprehensive worldwide with as many as 30 cells operating at a time and over 100 different experiments performed. The final result of this monumental effort in the words of the official press release was, "In none of these experiments was there any evidence of fusion taking place under electrochemical conditions. It should also be added that there was no evidence of excess heat generated by any of their cells".

Note that a disclosure in an application, to be complete, must contain such description and detail as to enable any person skilled in the art or science to which the invention pertains, to make and use the invention as of its filing date, In re Glass, 181 USPQ 31. Applicant's disclosure, however, does not contain the requisite description and detail.

Applicant's disclosure is insufficient and non-enabling as to how and in what manner, the invention can be carried out so as to provide an operative embodiment wherein useful amounts of removable heat are generated at and/or in the surface of the cathode (page 2 SUMMARY OF THE INVENTION of the specification and claim 1).

There is no adequate description nor enabling disclosure of all of the requisite parameters of a specific operative embodiment of the invention, including exact composition (including impurities and amounts thereof) of the electrolytic solution and of all components (such as the metal salts) therein; composition, size, dimensions of the

electrodes; the applied currents, current densities, and voltage; the instrument calibration prior to and during a run, test or experiment; etc.

Note that such parameters are critical in arriving at an operative cold fusion embodiment. For example, Morrison (VI)(Cold Fusion Update No. 8) shows the electrode spacing to be an important parameter. Page 3 thereof, shows that if the electrodes are close enough to each other, one can get recombination of hydrogen isotopes and oxygen (which can be misinterpreted as excess heat). Jones et al (I), Murray (III), Wilson et al, Lewis et al, Shelton et al, Shanahan (IV) are examples of documents showing the critical importance of proper instrument calibration and calorimetry analysis. Miles (C&EN, July 13, 1998 on pp 10, 11), Carr, Rolison et al, Green et al (particularly the second column on page 101), Williams et al, are examples showing the critical important of cell component composition and impurity content and of electrode pretreatment.

The specification (see for example pages 4+ and Figures 2-6) appears to refer to tests or experiments wherein nuclear reaction products such as excess heat were produced. However, these indications or allegations of the production of excess heat due to a nuclear reaction, are not sufficient to over come the numerous teachings by skilled artisans, (set forth both above and below by the examiner) that the allegations of the obtainment of nuclear reactions, excess heat, etc., in a cold fusion system are not reproducible or even obtainable.

Note that said "numerous teachings by skilled artisans" show that in this field, it is relatively easy to obtain false indications of positive results (such was also noted in the

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Nov. 1989 Report of the ERAB to the DOE, as well as in the DOE Dec. 1, 2004 "Report of the Review of Low Energy Nuclear Reactions" and by several of the Reviewers in the "2004 U.S. Department of Energy Cold Fusion Review Reviewers Comments").

Said "numerous teachings by skilled artisans" show how errors can arise in the detection of heat (or a temperature rise) in these cold fusion systems and, that such errors can lead one to the erroneous conclusion that excess heat is being generated (and consequently, that nuclear fusion reactions are taking place).

It is not clear from the information set forth in the specification, that when all possible sources of error are taken into account, that applicant would still be able to show positive results or that the alleged positive results do not fall within the limits of experimental error or are not a result of a misinterpretation of experimental results. Note in this respect, that the examiner has cited several documents that deal with calorimeter problems and other sources of error in cold fusion systems.

There are numerous external influences that can effect the measurement of any produced heat. Applicant's examples fail to set forth the error bars (and/or any cumulative errors) for the parameters and experimental equipment utilized in the determination of the excess heat or energy. Note for example, the analysis of calorimetric evidence for electrochemical induced cold fusion in MisKelly et al, Albagli et al, Lewis et al, and Ohashi et al.

It is also noted that applicant has only disclosed "a typical calibration" and "a typical result" **not ACTUAL results**. See, for example, the specification page 5 line 14 and 19.

Murray (VIII) refers to some of the problems with the use of isoperbolic calorimeters and indicates that such calorimeters appear to be unreliable for measuring excess heat in cells in which the electrode surfaces are changing (note that this is directly applicable to applicant's invention since applicants cathode surface is changing due to co-deposition of gold per the specification page 82 lines 4-5).

Miles et al (II) also state that reproducibility continues to be a major problem in "cold fusion", see the Abstract on page 5, which states:

"Reproducibility continues to be the major problem in this controversial research area. Based on our experiments, this lack of reproducibility stems from unknown variables in the palladium metal".

and on page 8:

"Most scientists hold the view that anomalous effects in deuterated metals can be explained by experimental errors. Some scientists go so far as attributing positive results to self-deception and even fraud and consign this phenomena to the realms of Langmuir's "Pathological Science." Due to the lack of experimental reproducibility, this field remains practically defenseless against such attacks. To our knowledge, no laboratory can provide detailed experimental instructions to another laboratory and guarantee the reproduction of the excess heat effect. Nevertheless, considerable knowledge has been gained concerning experimental conditions that favor the excess heat effect. Later in this report, we will provide a detailed description of procedures, based on our experiments, that favor the anomalous production of excess power. However, our experiments indicate that the lack of reproducibility is due largely to unknown and uncontrolled variables contained within the palladium stock. Studies by M. McKubre et al. at SRI have led to this same conclusion (Reference 1)." (Underlining added).

Buehler et al note some of the problems and errors that can occur in calorimetry and, outline some criteria for establishing calorimeter performance for more definitive measurement of excess heat.

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The Nov. 1989 Report of the ERAB to the DOE on pages 44+ sets forth some of the experimental problems in the evaluation of heat effects in cold fusion systems and, states that there was a lack of predictability and reproducibility (page 36).

The DOE Dec. 1, 2004 "Report of the Review of Low Energy Nuclear Reactions" under the heading "Charge Element 1" refers to some of the problems with excess heat measurements in cold fusion systems and indicates that most of the reviewers considered any positive excess heat results as not repeatable or reproducible.

Applicant states in the specification on page 2 that "This specification describes how such energy amplification conditions can be created at will, with complete reproducibility". However, with regard to the issue of reproducibility, note the following comments by Huizenga (IV) under the heading Reproducibility:

"The foundation of science requires experimental results to be reproducible. Validation is an integral part of the scientific process. Scientists are obligated to write articles in ways that allow observations to be replicated. Instructions should be available to permit a competent and well-equipped scientist to perform the experiment and obtain essentially the same results. Replication in science usually is reserved for experiments of special importance or experiments that conflict with an accepted body of work. The greater the implication of an experimental result, the more quickly it will be checked by other scientists.

As more and more groups at major universities and national laboratories were unable to replicate either the claimed excess heat or fusion products, proponents of cold fusion quickly pointed out that the experiment was not done properly: one needed different size palladium cathodes, longer electrolysis times, and higher currents, they claimed.

Whenever the inability of qualified scientists to repeat an experiment is met by ad hoc excuses, beware. One important role of a scientific article is to provide directions for others. Scientists establish priorities for their discoveries by publishing a clear and well documented recipe of their experimental procedures. If a scientific article fails to include an adequate recipe which

allows a skilled reader to reproduce the experiment, it is a warning that the author's understanding of their work is incomplete.

Cold-fusion proponents introduced new dimensions into the subject of reproducibility in science. Some tried to turn the table on reproducibility by giving irreproducibility a degree of respectability. A second aberration was to assign a different value to experiments attempting replication. Only experiments that obtained some fragmentary evidence for cold fusion were to be taken seriously because it was declared that experiments obtaining negative results required no special skills or expertise. This viewpoint led proponents of cold fusion to invite mainly papers reporting positive results when organizing conferences. Such an aberrant procedure is incompatible with the scientific process and usually is viewed negatively by scientists as well as journalists." (Underlining added).

Note that "reproducibility" must go beyond one's own lab. One must produce a set of instructions, a recipe, that would enable anymore in their own independent lab (including the labs of cold fusion skeptics), to produce the same results. If reproducibility only occurs in one's own lab, errors (such as systematic errors) would be suspect. See for example, Little et al.

As a further issue in regard to reproducibility, experimenters who previously found evidence of excess heat, found no evidence of excess heat when better calorimeter equipment was used (see section 2.2 on page 2 of Morrison (IV) (note that such refers to the work at IMRA (Japan))).

Reproducibility of the alleged positive cold fusion results is clearly a critical feature in determining if a disclosure adequately teaches the artisan how to make and use an invention for its disclosed purpose.

Accordingly, the logical conclusion when one does not get identical results and/or the results are not reproducible at will in these cold fusion experiments, is that the

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alleged positive results are not real but instead, they are due to experimental errors, instrumentation errors, misinterpretation of results, etc.

Clearly, when an artisan or experimenter is relying on the experimental results of a particular experiment(s) to establish certain facts, it is incumbent upon the experimenter to show that the alleged experimental results are valid and not just the result of experimental error (and that the alleged experimental results do not fall within the limits of experimental errors).

This is especially so when the experiments in question are (as here) in a field wherein the scientific community in general considers the alleged positive experimental results to be erroneous.

It is considered elementary that identical structures operated in identical manners, must produce identical results. Such is even relied on in one's everyday life.

If instrumentation, etc., indicates that identical structures operated in identical manners do not produce identical results, clearly, one of two things is implied:

1) The presumed identical structures actually are not identical, i.e. one of said structures actually has something additional, some critical feature not found in the other said structure, which causes said one of said structures to produce the positive results.

2) The structures actually are identical, however, instrumentation, etc. is producing spurious results leading to the erroneous conclusion that one or even some of said identical structures, are producing positive results.

If however, it is actually something additional, some critical feature, which causes some of these cold fusion systems to produce actual, positive results whereas otherwise

identical systems do not, then clearly, this something "additional", this critical feature, must be clearly specified so as to enable the artisan to make and use the inventions as required by statute.

In this same vein, note that Arnoldi et al. (U.S. Patent 3,440,153), Muraca (U.S. Patent 3,790,864) and Langley (U.S. Patent 4,512,866), each illustrate systems where a material is electrolytically deposited on a cathode. These three documents however, do not specifically disclose that such systems include the generation of excess heat.

Assuming for the sake of argument that applicant's invention actually is able to generate excess heat in contrast to the above noted systems of any of Arnoldi et al., Muraca and Langley. it can only be because of some undisclosed, additional, critical feature(s) not found in any of the above noted systems.

Applicants disclosure is hence insufficient and non-enabling in failing to disclose and identify said additional, critical feature(s) necessary to generate excess heat in contrast to the systems of any of Arnoldi et al., Muraca and Langley.

Applicant's specification indicates or at least implies, that applicant's invention generates heat energy in a manner, which does not emit harmful emissions.

Somewhat contradictory to the references, applicants state they have reported "solid evidence" of reproducible excess heat production.

Initially, it is noted that the failure to detect radiation (including neutron radiation) commensurate in scope with the alleged amount of excess heat being detected, has caused the scientific community in general, to take the position that the alleged reports of

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excess heat in cold fusion systems, are based on experimental error and/or misinterpretation of experimental results.

Huizenga (I) ("Cold Fusion: The Scientific Fiasco of the Century") may be resorted to for a showing that such radiation-less nuclear reactions are not scientifically feasible. Note particularly, pages 111-113 which describe the three "miracles" required for successful cold fusion (especially number 2 ("The Branching Ratio miracle") and number 3 ("The Concealed Nuclear Products miracle") (these miracles are further discussed in Huizenga (I) on pages 35-38, 130, 154, 160, 172, 173, 183, 207-208, 213, 217, 228).

The "Branching Ratio miracle" described on page 112 of Huizenga (I) requires the well-known branching ratios of the three reaction channels for D+D fusion (i.e. $D+D \longrightarrow {}^3He + n$, $D+D \longrightarrow T+p$, $D+D \longrightarrow {}^4He + \text{gamma ray}$, be highly modified such that only the reaction branch $D+D \longrightarrow {}^4He + \text{gamma ray}$, occurs in a metal lattice.

The "Concealed Nuclear Products miracle" described on page 113 requires that the 24 MeV of energy associated with the reaction branch $D+D \longrightarrow {}^4He + \text{gamma ray}$, be miraculously taken up by the lattice without producing detectable gamma rays.

Giglio provides a good explanation based on quantum mechanics and relativity, as to why this 24 MeV of energy associated with the reaction branch $D + D \longrightarrow {}^4He + \text{gamma ray}$, cannot be absorbed by the metal lattice and will instead be emitted as a 24 MeV gamma ray.

Note particularly, the following two quotes from "Message segment 3 of 3" of Giglio:

"Note also in this case, as well as the previous one, that the principles of quantum mechanics being invoked have been under development for most of this century. They have been tested in thousands of labs all over the world in many thousands of experiments, under the skeptical scrutiny of thousands of experimenters. If McKubre and other cold fusion advocates want their claims to be taken seriously, they will need to demolish this well-established and tightly interlocking body of theory and experimental observation. To do so will take more than just hand-waving and vague allusions to direct energy transfers by unspecified mechanisms -- mechanisms that remain unspecified after ten years of hand-waving."

"At the risk of being tiresome, I'll reiterate that the fundamental physics which cold fusion advocates dismiss so casually is not some religious or political ideology made up out of whole cloth. It is a massive, interlocking body of precise and CONSISTENTLY REPLICABLE experimental observations, associated with theories that possess extremely high levels of predictive accuracy, built up by thousands of workers in thousand of labs worldwide, upon which numerous powerful and precise technologies are based. The work started in the latter part of the 19th century and has occupied all of this one. It provides proof as solid as humanly achievable that the central claim of cold fusion (nuclear reactions can take place without producing "signature" particles and radiations) is simply dead wrong. And there's also an obvious corollary, namely that what the cold fusionists are measuring is nothing more than experimental error." (Underlining added).

This issue of the "Concealed Nuclear Products miracle" (as well as the "Branching Ratio miracle") was considered pertinent in 1989 as evidenced by the Nov. 1989 Report of the Energy Research Advisory Board (ERAB) to the DOE (e.g. see pages 15-19, 27, 28) and, is still considered pertinent at the present time, as evidenced for example by the DOE, Dec. 1, 2004 "Report of The Review of Low Energy Nuclear Reactions" and by some of the Reviewer comments in the "2004 U.S. Department of Energy Cold Fusion Review Reviewers Comments" (particularly Review #6 on page 10,

Review #15 on page 34 (second full paragraph), Review #17 (paragraph bridging pages 39, 40), Panel Review #18 (last paragraph on page 45).

As to further comments regarding the "Concealed Nuclear Products miracle", note particularly, the comments in Blue (II), Blue (III), Blue (IV) and Sullivan.

As noted above, said page 23 of said vol. 1 of Technical Report 1862 refers to applicants "solid evidence" of helium -4 production due to DD fusion reactions in the electrolytic Pd/D system.

However, the scientific community in general, considers the reported detection in such cold fusion systems of helium -4 due to DD fusion reaction, as being based on experimental error and/or misinterpretation of experimental results.

The problems with misinterpretation of helium measurements as evidence of nuclear fusion involving hydrogen isotopes goes way back, back to the 1920's, when Paneth and Peters initially reported the transformation of hydrogen to helium using a palladium catalyst.

Paneth (see Nature article dated 1927) subsequently retracted their earlier claim of using palladium to convert hydrogen to helium after determining that glass tubes readily absorbed helium from the atmosphere and that these glass tubes would release the absorbed helium when heated in the presence of hydrogen but not when heated in a vacuum or in oxygen.

Jone (I) and Jone (II) discuss this retraction of Paneth and Peters and, show how this release of absorbed helium from glass walls can account for the claims of Yamaguchi of producing helium in a cold fusion system. Jone (II) on page 2 states that

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deuterium has the property of heating rapidly upon expansion and thus, this may account for putative "heat bursts" in cold fusion experiments when deuterium is released for example from high-pressure pockets in the metal.

On page 127, Clarke (II) states that on the evidence, systematic error is the likely reason for the alleged observation of He-4 production by Dr. Case and by McKubre et al at SRI. Clarke et al (III) on page 254 state this systematic error may be due to SRI calibrating their mass spectrometer with deuterium gas having approximately 5ppm He-4.

Kunich shows that laboratories can have local concentrations of helium that can be orders of magnitude above normal background levels.

Murray (IV) (particularly pages 5 and 6) show how ICP-MS data can be misinterpreted as providing evidence of nuclear transmutations or transformation. Note also in this respect, the negative comments concerning nuclear transmutations in cold fusion cells, on pages 7 and 8 of Morrison (IV).

Note the statements (reproduced below) concerning nuclear transmutation on page 1 of Segment 2 of Bass.

"Scott's second epistle recapitulates what we learned from realms of data from Miley's and the Cincinnati Group's transmutation data. No reasonable evidence for transmutation exists once allowance is made for the innumerable ways complicated and subtle instrumentation can be wrong. You see, we can't even determine with those remarkable systems something as simple as 10 ppm Zn in pure Li₂SO₄. Prejudiced and desperate attempts to quickly survey complex unknown samples results in "data stew". (Underlining added).

Note also that page 2 of Segment I of Bass indicates that errors can easily occur in ICP/MS when working on unknown and/or unusual samples. Said page 2 states that different labs using samples split from the same regent grade Li₂SO₄ came up with

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differing amounts of Zn as being present in the Li₂SO₄. See the fifth paragraph on page 2 which states:

"The Aldrich analysis showed 4 ppm Zn. The old lab got 9 ppm Zn. The new lab got 51 ppm Zn. I told the new lab what the other two results were and asked them to repeat their analysis, they managed to come up with 31 ppm Zn the second time".

For additional negative comments on the alleged transmutation of isotopes in a cold fusion cell, note Jones et al (IV) states on page 146 that radioisotope contaminants can be present in the electrolyte, electrodes or other components and; note also pages 152-156, 237, 269, 275, 276, 284, 286 of Huizenga (I). Said pages 152-156 indicate that some experimenters at the Naval Research Laboratory had mistakenly reported the production of particular palladium isotopes by neutron transmutation in cold fusion cells using a technique known as SIMS (secondary ion mass spectroscopy). Said pages 153 to 156 set forth reasons for this misinterpretation of experimental data. Note said page 156 which states:

"The story associated with the palladium isotope anomaly is not nearly so interesting because it is was simply due to an erroneous interpretation of data where the experimental mass peaks were misidentified. Contributions from polyatomic species of impurities with masses nearly coincident with those of the palladium isotopes caused the misidentification.

In spite of the fact that the palladium isotope anomalies had been discredited for over five months, Bockris submitted a paper on March 26, 1990 [Fusion Technology 1811 (1990)] in which he discussed, along with other cold fusion phenomena, the thermal and 14-MeV-neutron-induced cross sections on palladium isotopes. He used these mistaken isotopic anomalies data to suggest that the cold fusion reaction is a surface or near-surface reaction, and, therefore, to serve as supporting evidence for his model of fusion. Among cold fusion enthusiasts mistakes and erroneous results usually decay with a very long lifetime". (Underlining added).

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Note further, that there are cold fusion experimenters (especially those who believe in cold fusion) who consider that it is specific impurities and the level of these impurities, that cause some Pd metals to produce or catalyze nuclear fusion while other Pd metals (even some from the same supplier and batch) do not cause nuclear fusion to take place.

Note for example, Murray (I) on page 1 quoting Edmund Storms:

"...only certain samples out of the same batch of catalyst work. Presumably if the He concentration were uniform, all samples would show He production. On the other hand, failure to initiate a nuclear reaction could result from failure to remove all impurities from the surface in the failed runs. This purification process is known to be important and tricky."

See also the following statement by M. Miles on page 10 of the 7/13/98 issue of

C & EN:

"The cold fusion controversy will continue until an experiment is so clearly defined that it can be readily reproduced in any laboratory. My results, along with the reports from many other laboratories, suggest that there are hidden variables within the palladium metal that are not yet under experimental control. These variables include the grain size and impurity levels...." (Underlining added).

For further documents illustrating this problem of finding an appropriate or suitable Pd material, note Murray (III) and Carr.

Pages 7+ of Morrison (VIII) refers to various government agencies (both U.S. and foreign) as well as companies, etc., that have stopped funding cold fusion research, with page 8 estimating a lower limit of a hundred million dollars as having already been devoted to cold fusion research.

It does not seem plausible that all of these government agencies, companies, etc., would cut off funding for cold fusion (especially after an overall outlay for cold

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fusion research of an estimated lower limit of a hundred million dollars), when there already exists a cold fusion system such as applicants cold fusion system, which is capable of reproducibly producing excess heat and tritium.

What is more plausible is that the funding was cut off because these cold fusion systems (including that of applicants) actually could not reproducibly produce excess heat and tritium.

Indeed, this is the position taken in the Dec. 1, 2004 DOE Report (Report of the Review of Low Energy Nuclear Reactions), even though the Reviewers for this report had been made aware of applicants cold fusion experiments.

Note that the examiner has cited documents showing how easily, experimental data, experimental errors, etc., can be misinterpreted as providing evidence of the operability of cold fusion systems and that the scientific community in general does not consider such cold fusion systems real, valid or operative.

In this respect that there has essentially been a continuing stream of publications from 1989 on showing that virtually none of the scientific community consider the alleged positive results of these "cold fusion" experiments, as being confirmed. In this respect, attention is directed to Merriman et al, Ewing et al, Bosch et al, Fleming et al, Balke et al, Henderson et al, Nova, Huizenga (I), Huizenga (II), Huizenga (III), Huizenga (IV), and Rogers et al.

These references provide further clear evidence that no excess heat is generated in such "cold fusion" system nor is there any evidence of nuclear reactions or transformations taking place.

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As to some of the Japanese claims of positive cold fusion results, note the comments by David Williams in the Hadfield article on page 10 of the 10/31/92 issue of New Scientist. David Williams (head of the department of chemistry at University College London) described the claims as "absolutely pie-in-the-sky".

Note also the negative comments in Huizenga (I) as to some of the Japanese work in cold fusion alleging positive results (e.g. see pages 240, 246, 251, 252, 277-281).

Williams et al, Broad and NOVA refer to some of the spurious effects, faulty data, etc., which have led to some of the claims of the existence of cold fusion.

Clark et al (IV) show that metals can become contaminated with tritium from the atmosphere, that the amounts of absorbed tritium can be higher if the metals were close to releases of tritium by industry or by thermonuclear testing and, that such can lead to erroneous cold fusion results.

As to further problems with the use of detection of tritium as evidence of cold fusion, Kay et al show that ion-molecule reactions in the ionizer of a mass spectrometer can form the stable triatomic ions HD_2^+ and D_2^+ which can be confused with DT^+ and T_2^+ .

Brudanin et al also present various manners in which false indications of tritium and neutron production can occur in cold fusion experiments and, which must therefor be accounted for.

Likewise, Wolf et al set forth several possible causes of spurious detector signals and of various factors which can be mis-interpreted as evidence of tritium production.

Attas et al on page 390 of Nature, vol. 344 shows how solar flares can account for the alleged production of neutrons in cold fusion systems.

The Dagani article in the 1/14/91 issue of C & EN states that the "cold fusion" claims are taken seriously by virtually none of the scientific community and that research at Utah's National Cold Fusion Institute (NCFI) as well as research elsewhere, have failed to establish the existence of cold fusion.

Another article by Dagani (in the 6/14/93 issue of C & EN) entitled "Latest Cold Fusion Results Fail to Win over Skeptics", states that "the vast majority of scientists...dismissed the evidence of nuclear fusion results inside a metal lattice as nonsense-a case study in pathological science".

Note particularly the excerpts from the book "Too Hot To Handle" by Frank Close. This book refers to various errors in the work of F and P (e.g. see pages 161+), as well as by other experimenters (note particularly the comments on excess heat in calorimetry on pages 351-353).

In this same vein, note the analysis of calorimetry with electrolytic cells of the F and P type, set forth in Wilson et al, as well as the comments concerning possible errors in heat measurement by Jones (on pages 284, 285 of Surface and Coatings Technology) and, by Albagli et al.

Hilts states that the MIT experiments failed to produce any of the excess heat reported by the Utah group.

Lewis et al state in the summary on page 525 that they found no evidence of excess enthalpy in their experiments and, they refer to various possible sources of error which could lead to the erroneous conclusion that excess heat was produced (note pages 528-530).

Hilts, Lewis et al and the DOE Dec. 1, 2004 "Report of the Review of Low Energy Nuclear Reactions", indicate that in any determination of excess heat, one must determine the total amount of energy produced (as heat and chemical energy) integrated over the whole period of cell operation, versus the total energy input.

Another document showing how experimental data, etc., can be misinterpreted as providing evidence of the operability of cold fusion systems, is the transcript of the television show on NOVA entitled, "Confusion in a Jar", which indicated that in these cold fusion experiments, it is fairly easy to get quick results which could be "interpreted" as providing evidence of "cold fusion" but that in very carefully run experiments which were rechecked, etc., such as by using several different methods and/or detectors to attempt to detect the same presumed experimental results, the end result was negative.

The article by Taubes on pages 1299-1304 of the 6/15/90 issue of Science, explains why the alleged detection of tritium at Texas A & M cannot be relied on as evidence of "cold fusion" actually taking place.

Note that evidence shows that even having a high concentration of deuterium in the cathode will not result in nuclear fusion taking place.

In this respect, Silvera et al found no evidence of "cold fusion" with a D/Pd ratio as high as 1.34 and, Myers et al obtained negative results even with a D/Pd ratio as high as 1.6.

Dagani in the June 5, 1995 issue of C & EN refers to experimental errors which negate the positive results of some cold fusion experimenters.

In a 1992 article in Surface and Coatings Technology, Jones takes the position that the claims of excess heat, tritium and helium production due to nuclear reactions are "dubious to say the least" (note page 288) because there is no evidence of commensurate nuclear products. Note the reference to $E=mc^2$ on page 286.

Taubes, "Bad Science: The Short Life and Weird Times of Cold Fusion", 1993, is a good reference for showing the view point of the scientific majority towards cold fusion. After interviewing over 250 people in the field, Taubes concluded that "Cold Fusion... does not exist", and "As long as financial support could be found, the research would continue... In fact, the few researchers still working in the field would have little incentive to acknowledge negative results as valid, because such recognition would only cut off their funds". Note page 426.

Another good reference presenting a compilation and analysis of cold fusion work subsequent to the 1989 announcement of the cold fusion claims of F and P, is the book, "Cold Fusion: The Scientific Fiasco of the Century", by Huizenga (I). Huizenga was co-chairman of the Nov. 1989 DOE/ERAB panel on cold fusion. Note particularly the "Epilogue" on pages 237-287 which discusses some of the alleged positive results presented at the First, Second and Third Conferences on Cold Fusion.

On pages 201+ (and more particularly, page 214) Huizenga (I) indicates cold fusion can qualify or be characterized as "pathological science", defined as "the science of things that aren't so" (see also Huizenga (II), Huizenga (IV), Morrison (II), and Rousseau in this respect).

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On page 206, Huizenga (I) states that some of the similarities between cold fusion and other unsubstantiated concepts, are

- (1) lack of control experiments,
- (2) statistical uncertainties,
- (3) irreproducibility and
- (4) the public description as a "simple experiment".

Note particularly pages 125, 222, 223, of Huizenga (I) which refer to the lack of reproducibility of the alleged "positive" cold fusion results.

Clearly, if something cannot be reproduced at will, there is also, then, no enabling disclosure which would enable one of ordinary skill in the art, to make and use it, as required by statute (35 USC 112).

Morrison (I) in Trans. of Fusion Technology, sets forth various criteria to be followed in doing cold fusion experiments and of problems that can arise.

Jones et al (II) and Jones et al (I) debunk the positive cold fusion claims of Miles et al at the Naval Research Lab in China Lake, showing how experimental errors, etc., can give a false impression of positive results.

Jones et al (III) and Shkedi et al show how faradaic efficiencies of less than 100% during electrolysis of water can account for alleged reports of excess heat in "cold fusion" cells.

For a good discussion of errors arising in cold fusion tests or experiments, note for example, the book, "Too Hot To Handle", by Frank Close. Pages 259-263 of this book set forth various errors that can occur, leading to the erroneous conclusion that

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excess power was produced in the cold fusion experiments. Page 261 of this book contains the telling statement:

"In addition to these experimental problems there were several examples where the numeral evaluation of the data and assessment of error were incorrectly or badly done or, in some cases, not done at all..."

The DOE panel commented that there had been a noticeable lack of attention to the statistical assessment of errors, and that in some cases, where heat was being claimed, a group's claim of excess heat is not supported with results of sufficient precision to allow such a conclusion. More usually it is not possible to assess precision from reported results because the result is reported from a single run and no error bars are provided for the measured parameters....

The DOE panel noted: 'Conclusions in this area simply cannot be accepted without a thorough assessment of the measurement errors. In its visits and conversations the members of the panel were struck repeatedly by the absence of critical assessments of this kind.'" (Underlining added).

In this respect, Morrison (III), Jones et al (I), Murray (II), Murray (VI), Jones et al (II), Jones et al (III), Green et al, Shelton et al, and Merriman et al, discuss some of the possible sources of errors (including systematic errors) in the calorimetry that can lead to the erroneous conclusion that excess heat was present. As indicated above, Buehler et al set forth criteria useful for establishing calorimeter performance for measurements of excess heat. Murray (VII) lists several questions to be addressed in cold fusion calorimetry so as to provide more accurate results. Murray (VI) states subtle systematic errors cannot be found by analyzing the final report of an experiment, since by that time any inconsistencies that might have pointed to such an error have been smoothed over and cultivated out of the data and the only way to find such errors is to immerse yourself in the laboratory with the working experiment and just go over everything countless times.

As to further documents illustrating errors that can occur and/or have not been accounted for in cold fusion experiments, see J.E. Jones, Giglio, Shanahan (II), Shanahan (III), Schultz (II), Blue (I), Carr, Hoffman, Shkedi et al, Shelton et al, Jones et al (III).

It is the examiners' position that an undue amount of experimentation would be required to produce an operative embodiment of applicant's invention. The examiner has cited numerous documents showing that experimenters have obtained negative results using various types of cold fusion apparatus, all based on the cold fusion concept set forth by F and P. These documents show how easily experimental results can be misinterpreted as evidence of cold nuclear fusion.

This issue of undue experimentation has been succinctly addressed by Douglass Morrison at the Fourth International Conference on Cold Fusion Technology, (ICCF-4) held Dec. 6-9, 1993 in Hawaii (reproduced in Transactions of Fusion Technology vol. 26, Dec. 1994), see page 54 which states:

"IX. When A COLD FUSION WORKING DEVICE?

8 December 1993; the previous speaker, Dr. H. Fox, giving he said, a businessman's point of view, declared he expected a working Cold Fusion device in 20 years.

November 1993. Dr. S. Pons said that by the year 2000 there should be a household power plant -6 years.

1992. Dr. M. Fleischmann said a 10 to 20 Kilowatt power plant should be operational in one year.

July 1989, The Desert News published an article by JoAnn Jacobsen-Wells who interviewed Dr. S. Pons. There is photograph in color, of Dr. Pons beside a simple apparatus with two tubes, one for cold water in and one for hot water out. This working unit based on Cold Fusion was described as "It couldn't take care of the family's electrical needs, but it certainly could provide them with hot water year-round" said Pons".

Later in the article it was written "Simply put, in its current state, it could provide boiling water for a cup of tea."

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Time delay to this working model – Zero years.

Thus it appears that as time passes, the delay to realization of a working model increases.

X. CONCLUSION

No conclusions are presented – everyone can judge for themselves. However some questions can be asked;

Are Cold Fusion results consistent in claiming Cold Fusion effects in Deuterium but not in normal Hydrogen, while other groups claim Cold Fusion effects with hydrogen?

Is the ratio of tritium to neutron production about unity as Fleischmann and Pons originally claimed [5] or is the ratio in the wide range $10^4 - 10^9$ as most other workers claim?

Are transmutation, Black Holes, Biology [18] part of the normal world of Cold Fusion?

To explain the null experiments there is one theory – the conventional theory of Quantum Mechanics, but there are a wide variety of theories to explain positive Cold Fusion results – can they all be valid simultaneously – if not, which should be rejected?" (Underlining added).

For a further discussion of the contradictory results of different cold fusion experiments as well as with established laws of Nature, note the comments in Sullivan and in Morrison (VIII). Note particularly the "Conclusions" and the list of 9 various results of cold fusion experiments, reproduced herein from pages 20, 21 of Morrison (VIII):

"I have often looked at experiments which gave results that appeared to violate the laws of Nature which had been established by previous work. Later these experiments turned out to be false, but I have often found it very difficult to see just where the error was. But the fact that I had not detected the flaw, did not mean that the experiment was correct and that the laws of Nature had been violated.

Rather I feel the same as being at a circus watching a magician. Normally he and I know that the laws of nature are being obeyed but there is a trick which is hard to spot. At trick one, I may spot the trick and am happy that there is no problem with the laws of Nature – similarly with trick number two. But suppose at trick three, I do not see how the magic is performed. The magician may say "I won, I tricked you" and it is left unsaid that the laws of nature have not been violated. But suppose the magician says "You did not see anything wrong with my demonstration, therefore it is true. See, I have supernatural powers. The old laws of Nature have been replaced by new laws". And if I protest, I am told that I

have a closed mind, am an establishment figure, and do not face up to the happening performed in front of me. But almost all magicians admit that it is all trickery and the laws of nature are not threatened.

So if someone comes along and says, "Look – excess heat – do you see anything wrong?", then I feel as if I am at the circus, and although I do not immediately see anything wrong, I am reluctant to give up well-established laws of Nature unless the proof is very strong. Here reports on cold fusion happenings are described, especially in the summary talks by True Believers in cold fusion in their words, and then some clues as to possible explanations are offered. How many Elvis sightings constitute a proof?...

APPENDIX 1 – PROBLEM FOR EDWARD TELLER

Back in 1992, Edward Teller attended a private meeting on cold fusion in Washington. He delighted the media-aware people, e.g. Mallove, by proposing a new particle which would explain the contradictions of the then cold fusion results – how to have lots of excess heat without commensurate production of protons, neutrons, ^3He , tritium and gammas. When I phoned him, he explained that the clue was in the name of this hypothetical particle which in his native Hungarian means "Crazy". Little was heard of this afterwards.

At the 2000 meeting in Lerici, a friend of Teller attended. If he were to list the properties required of another new hypothetical particle that could explain all the various results of cold fusion experimenters, then the list of requirements would look something like this;

1. Gives heat of cold fusion at a rate 10^{40} times more than expected from potential barrier considerations
2. Gives excess heat in cold fusion in both light hydrogen and in deuterium
3. This excess heat should give some ^4He and possibly some tritium but no protons, no neutrons (except in certain labs), no ^3He (except in certain labs) and no gamma rays of 24 MeV.
4. When the fusion takes place in palladium, X-rays of 21 KeV, characteristic of palladium, should not be observed.
5. Transmutations should occur on electrolysis, mainly into stable ground states, but not into radioactive isotopes.
6. These positive fusion and transmutation processes should only occur with very small quantities of material, typically 40 milligrams, but not in bulk material.
7. Transmutations and excess heat should also be observed when there is no metallic crystalline structure (i.e. no coherence effects)
8. The cold fusion should occur at both low loading, e.g. by gas, as well as high loading of hydrogen into the electrode. But at very high loadings, obtained using a diamond anvil, no excess heat is produced.
9. Biological transmutations should also occur
10. Alchemy should occur but most strongly in the time window between 25 March and 15 June.

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Morrison (V) provides a good report on the 5th Cold Fusion Conference and Morrison (IV) (discussed above) provides a good report on the Sixth Cold Fusion Conference.

Note also the negative comments concerning "cold fusion" in Hoffman. See particularly page x of the Forward in Hoffman wherein Dr. T. R. Schneider of EPRI states:

"Where do these various efforts stand today? In my personal opinion, the overall finding is negative; that is, no verifiable evidence exists for nuclear effects consistent with the claimed "excess heat" measurements. Indeed the lack of any significant measurements of nuclear products suggests that the proponents' interpretation of the anomalous heat as real, yet unexplainable by any chemical, electrical, or mechanical source and hence by implication a nuclear phenomenon seems to me to be, at best, an extremely naive interpretation and reflects a very poor understanding of modern scientific method. The alternative explanation, that the anomalous heat measurements are not from nuclear reactions but are the result of an unidentified error or artifact, appears to me to be the only viable explanation of the "excess heat.""

... "Frequently, when the experiment has been improved to avoid possible artifacts, the measured quantity is reduced in intensity or even disappears". (Underlining added).

For a good overall analysis of the status of Cold Fusion/Low Energy Nuclear Transmutations (CF/LENT), attention is directed to the MEMO (dated 10/9/97) from Bennett Miller to Dr. Robert W. Bass.

The Miller Memo indicates Dr. Bass had requested the Department of Energy to do a new, full-scale review of the Cold Fusion/Low Energy Nuclear Transmutations (CF/LENT) phenomena because of what Dr. Bass considered to be "emerging evidence of progress".

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The Miller Memo indicates DOE's response was to commission Mr. Miller to do the review.

Page 3 of the Miller Memo indicates the vast amount of documents, etc., reviewed and considered by Miller in arriving at his conclusions.

Basically, the conclusion of the Miller Memo is that there is still no concrete evidence of excess heat, nuclear transmutations, etc. Note particularly the following excepts from the Miller Memo:

"The core problem that I have with CF/LENT is the disconnect between the public pronouncements of its proponents regarding the imminent commercial availability (nay, already established commercial availability if I am to believe the press clippings) of such systems and the somewhat more private and negative developments that seem to emerge at every turn.

Most prominent, but still only three among many such examples of the former, are first, the Cincinnati Group's recent representations regarding a revolutionary approach to the nuclear waste remediation problem – representations that you openly endorsed as revealed truth; second CETI's equally bold guarantee of a CF cell that put out aneutronic, excess heat on a reliable, predictable basis. And, third, your vouching to me, some time ago, for the imminent commercial installation operation of a CF power system in a hotel/resort complex that is currently under construction.

Moreover, the casual reader, picking up an issue of Infinite Energy, for example, would be hard pressed not to conclude that CF/LENT is a closed matter as far as demonstrating scientific feasibility is concerned. Around the world, governments and industries are successfully demonstrating the phenomena of excess heat, at the very least. If so, no further development, let alone research, is needed or desired. What possible role should or could your government's federal research and development community play when its charter is to support primarily that work that the private sector cannot or will not do on its own?

At the same time of course, more careful attention to what is going on suggests that not all is what it seems to be. The CG approach to nuclear transmutation is at best mired in controversy of the most basic sort. There is no verification of initial claims. There is no explanation of the basic process. A recent attempt to verify the process by a third party in one of DOE's national laboratories, was, in everyone's opinion, a failure; though it can be argued that the tests were inconclusive for a number of reasons. The CETI cell has similar problems. The hotel project with the 500 kw CF power plant, about which you

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were so enthusiastic, has been delayed indefinitely. And, the Japanese have terminated their three-year million dollar effort to demonstrate and commercialize cold fusion.

Perhaps this evidence that all is not well can be explained by sloppy science, or just complicated science, or financial difficulties unrelated to science, or by government mismanagement, or by pressure to move in different directions, though in the case of Japan that is hard to believe. Your assertion that the Japanese government has applied pressure internally to disband the effort flies in the face of all logic.

If any nation accords energy matters a higher priority than the Japanese, I do not know of it. If cold fusion is real, demonstrable, and reproducible it would mean more to the Japanese than any other industrialized nation. It would be a harbinger of the ultimate energy security that they have been seeking for the past 70 years-a security of energy supply that was one, if not the most, important determinant of their willingness to go to war in 1941. What possible motive could be to disbanding an effort that advocates of CF/LENT expected to succeed, except that perhaps it was not?

In fact it is my current understanding that the NHE program was disbanded precisely because it could not meet its primary objective of a concrete demonstration of excess heat, even after three years of work and an expenditure of over \$30 million. There have been claims made that the efforts was poorly managed -- that emphasis was incorrectly given to building a precommercial infrastructure at the expense of doing the science that needed to be done. If so, that is truly a sad state of affairs. But if it is true, I believe it will be corrected in fairly short order if for no other reason than that the stakes are so large. Nonetheless, the effort by a major industrial nation to amount a successful, ministry-sponsored, CF program cannot be characterized as anything other than a failure at this point.

This line of inquiry bring us back to the fundamental dilemma. If CF/LENT is as real as some of the scientific results presented at respected scientific meetings (or as real as its press clippings), then it is already well beyond the stage where federal tax dollars are needed. It is a commercial reality, or so close that the private sector should be jumping at the business opportunity of a lifetime – the opportunity to capitalize on a discovery of momentous proportions that is relatively uncluttered by government claims to prior knowledge or prior invention.

If on the other hand, CF/LENT is still in that nascent stage where nothing is really clear and where the prospect still exists that all is artifact and anecdote, then there is only one prudent course for practitioners to follow -- go back to basics and systematically subject the phenomena to careful examination by the time-tested process of merit-based, peer-review.

I believe, as I have already stated, that I think there are good things to be done in this arena. New ground to be broken. New discoveries to be made. New industries created. But only after the basic science has been illuminated and accepted by the scientific community at large. That is how we, as a nation,

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have built the greatest scientific establishment in the world. I urge you and your colleagues to accept the challenge. Come forward. Present proposals. Abide by the process.

It will not be easy. Nor will it be guaranteed of success."

Note that Blue (I) (like the Miller Memo above), also refers to Japan as dropping the funding for cold fusion research.

Chechin et al in Inter. J. of Theoretical Physics and Lindley in vol. 344 of Nature, provide a critical review of numerous theoretical cold fusion models, showing why these cold fusion models are not valid. Chechin et al debunk various cold fusion theories including the theories of Chubb and Chubb (pp. 635-636) and Hagelstein (pp 638-639).

Fukai shows that deuterium nuclei in a palladium lattice cannot come close enough to each other to undergo nuclear fusion and, that the electrons do not provide an effective screening.

There have been allegations of excess heat in "boil dry" or "heat after death" experiments.

However, the alleged showing of excess heat such as in "boil dry" or "heat after death", experiments such as that of F and P are of no merit in view of the showings for example, in any of Morrison (III) (Physics Letters A), Morrison (VI) (Cold Fusion Update No. 8), Morrison (VII) (Cold Fusion Update No. 9), Droege (I) (Maui Paper #4), White, Wilson et al.

As to the purported positive results of some specific artisans in the cold fusion field, note that Jones (Surface and Coating Technology – 1992), Jones et al (I) (J. Phys.

Chem; 1995), Jones et al (III) (J. Phys. Chem. B, 1998), Droege (I) (Maui Papers #4), show that the alleged positive results of Dr. Miles cannot be relied on as accurate.

Even Miles himself in an article entitled "Cold Fusion Controversy" on pages 10, 11 of C & EN (July 13, 1998) states:

"The cold fusion controversy will continue until an experiment is so clearly defined that it can be readily reproduced in any laboratory. My results, along with the reports from many other laboratories, suggest that there are hidden variables within the palladium metal that are not yet under experimental control. These variables include the grain size and impurity levels."

Szpak et al (I) also state on page 241 that there are "hidden variables" in such cold fusion electrolytic systems.

Miles et al (II) also refer to "unknown variables" causing a lack of reproducibility (e.g. see pages 5, 7, 8, 10).

Little (I) (21 May 1998), found no evidence of excess heat when attempting to duplicate the alleged positive results of Dr. Case, even with input from Dr. Case. Clarke (II) (Jan. 2003) also found no evidence of He-4 production from DD fusion in Case-type cells. On page 127, Clarke (II) states that on the evidence, systematic error is the likely reason for the alleged observation of He-4 production by Dr. Case and by McKubre et al at SRI. Clarke et al (III) on page 254 state this systematic error may be due to SRI calibrating their mass spectrometer with deuterium gas having approximately 5ppm He-4.

As to Dr. Arata's alleged positive results, note for example, the negative comments in Murray (VII), Shanahan (I), Shanahan (II), Clarke (I), Clarke et al (I), Clarke (III) and Clarke et al (II). In the second column on page 152, Clarke (III) concludes that Arata et al actually recorded the well-known (to mass spectroscopists) instrumental

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"memory effect" for He-4 and mistakenly labeled it as a genuine signal of that isotope in the Pd-black samples. Clark (III) states that in this type of "memory", a D₂⁺ ion beam is much more effective in releasing imbedded He-4 (from previous samples) from interior sections of the mass spectrometer than an H₂⁺ ion beam. Clarke et al (I) end with the telling statement, "To be sure, it is very attractive to consider the prospect of energy produced by "cold fusion"; however, we are not willing to suspend the laws of nature (as the ancient Greeks said it) in order to "save the phenomenon"."

As to Dr. McKubre's alleged positive results, note for example, the negative comments in Shelton et al, Green et al, Shkedi et al, Giglio, Murray (II), Murray (V), Shanahan (III).

Note particularly, the following quote from McKubre in Murray (II):

"We do not know how to reproduce our own experiments. We have generated more null results and hours of beautiful calorimetric balance (>100, 000h) than anyone on the planet except Fleischmann and Pons. Nevertheless, the existence of a thermal anomaly in the D/Pd system is clear to me, as it is to them because we have seen the effect with our own eyes and modulated it with our own hands. We cannot prove it to you because we are not in control of all critical parameters. You should be skeptical, and remain so until we supply proof". (Underlining added).

Shanahan (III) (dated 2003) refers to problems with the helium measurements by McKubre at SRI, (said problems including significant contamination due to leakage to air). The problems with leakage to air at SRI, are discussed in detail in Clarke et al (III). Giglio shows that if McKubre is actually producing helium, he should also be detecting gamma radiation and, shows that the energy from the alleged nuclear fusion reactions cannot be transferred to the cathode crystal lattice as speculated by some cold fusion

advocates.

Shanahan (IV) (Themochimica Acta (2002)) and Shanahan (VIII) refer to a systematic error in mass flow calorimetry that can account for the alleged excess power (heat) results in cold fusion experiments. Shanahan (V) (dated 2002) states that the recent Navy report also did not consider this systematic error and so, does not eliminate it is a possible explanation.

In this same vein, Shanahan (VI) discusses a report by Szpak, Mosier-Boss, Miles and Fleischmann in which they attempt to reject recombination as the actual cause of their alleged excess heat observations (see also Shanahan (VII)). Shanahan (VI) states that Szpak et al have misunderstood the at-the-electrode, under the electrolyte surface recombination issue and that their prior photographic evidence is in fact, reasonable evidence of this recombination phenomenon.

Dewey on page 996 of Electronics World and Wireless World states that this hydrogen-oxygen recombination can cause "hot spots" on the cathode, thus also supporting Shanahan (VI).

Shanahan (VI) sets forth physical conditions, which could produce a calibration constant shift, and what might cause those conditions to arise. Note particularly, the "Conclusions" in Shanahan (VI).

The documents relied on above by the examiner, are evidence that one of ordinary skill in this art does not know the parameters of an operative cold nuclear fusion/cold nuclear transformation system which is actually capable of producing nuclear reactions or excess heat, nor, how to determine these parameters and, do not consider

such cold nuclear reaction systems to be operative.

This being the case, it is necessary for applicant's specification to disclose the requisite parameters for obtaining the particular disclosed and claimed nuclear reactions, nuclear transformations, and/or heat energy, when utilizing applicants particular cold nuclear reaction system.

However, applicants specification is insufficient and non-enabling in failing to set forth said requisite parameters.

In the present case, the examiner has stated above that there is no adequate description nor enabling disclosure of said requisite parameters.

Note again that the examiner has presented extensive documentary evidence that those of ordinary skill in this art do not know what specific parameters, are actually necessary to cause the production of nuclear reactions or excess heat in this type of system. See Bank v. Rauland Corp. 64 USPQ 93; In re Corneil et al., 145 USPQ 697.

Note further that said extensive documentary evidence shows that the scientific community in general considers the alleged positive cold nuclear reaction results to be no more than the result of experimental errors or misinterpretation of experimental data, and not-reproducible.

Again, as set forth above, "reproducibility" must go beyond one's own lab. One must produce a set of instructions, a recipe, that would enable any one in their own independent lab, to produce the same results. If reproducibility only occurs in one's own lab, errors (such as systematic errors or misinterpretation of experimental data) would be suspect.

The present case is considered analogous to that in In re Chilowsky, 134 USPQ 515, wherein the Court held the disclosure to be insufficient. In the present case, the examiner has shown that various necessary parameters have not been provided and, the examiner has provided evidence that the artisan does not know the requisite parameters of an operative cold fusion system, nor how to make an operative cold fusion system.

Note in this respect the Court's statement on page 519 of In re Chilowsky:

"Chilowsky could not start to describe his invention with the assumption that those skilled in the art knew in detail how to build his nuclear reactor. Since it was a major part of what he purported to have invented, it is incumbent on him, under section 112, to tell how to build it, under principles of patent law too elementary to require discussion".

It is apparent from the specification that applicant's concept or theory of obtaining an operative cold fusion system, is actually based in part on his own theories and speculations and on the "cold fusion/nuclear reaction" systems that came about from the work of F and P, and it is workable or operative, only if these systems are already operative.

However, as set forth above, the examiner has presented evidence showing that in such cold fusion systems, the claims of nuclear reactions or excess heat (as well as of other nuclear reaction products), are not reproducible or even obtainable. In fact, the examiner has presented documentary evidence disputing the instant applicant's own theories. It consequently must follow that the claims of nuclear reactions or excess heat are not reproducible or even obtainable with applicant's invention. While applicant may have set forth theoretical concepts, it is well known in the cold fusion/nuclear reaction field that theory and reality have a habit of not coinciding. Even applicants acknowledge

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the controversy and negative findings throughout the specification, see, for example, page 1 of the specification. There is no evidence to indicate applicant has so succeeded where others have failed, in arriving at an operative cold nuclear reaction system, i.e. that he has progressed his system beyond the point of an unproven theory or concept which still requires an undue amount of experimentation to enable the artisan to make and use the inventive system for its indicated purpose. This view is also considered supported by the failure to set forth a full example of the specific parameters of an operative embodiment. One cannot rely on the skill in the art for the selection of the proper quantitative values to present an operative cold fusion system, since those in the art do not know what these values would be. See Bank v. Rauland Corp., 64 U.S.P.Q 93; In re Corneil et al., 145 U.S.Q. 697.

It is thus considered that the examiner (for the reasons set forth above) has set forth a reasonable and sufficient basis for challenging the adequacy of the disclosure. The statute requires the application itself to inform, not to direct others to find out for themselves; In re Gardener et al., 166 USPQ 138, In re Scarbrough, 183 USPQ 298. Note that the disclosure must enable a person skilled in the art, to practice the invention without having to design structure, not shown to be readily available in the art; In re Hirsch, 131 USPQ 198.

To comply with the enablement requirement of the first paragraph of 35 USC 112, a disclosure must adequately present the claimed invention so that an artisan could practice it without undue experimentation. In determining whether any given disclosure would require undue experimentation to make and use the claimed subject matter,

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consideration must be given to such factors as the relative skill of those in the art, the state of the prior art, the nature of the invention, the presence or absence of a working example, the amount of direction or guidance presented, the predictability or unpredictability of the art, and the quantity of experimentation necessary. Ex parte Forman 230 USPQ 546, 547. Note that the examiner has taken each of these factors into consideration and, based on the extensive documentation set forth above, does not consider applicants disclosure as complying with the enablement requirement of 35 USC 112, first paragraph.

Additionally, it is noted that there has been a published Board decision involving "cold fusion":

See Ex parte Dash, 27 USPQ 2d 1481, wherein it was held that the examiner did not err in rejecting claims for "cold fusion" of nuclear energy for lack of enablement under 35 U.S.C. 112 and as inoperative and lacking utility under 35 U.S.C. 101, since evidence demonstrating that neither excess heat nor traditional nuclear by products of fusion reaction have been detected by careful researchers conducting experiments under conditions that are highly analogous to applicant's electrolytic cell, and demonstrating relative ease with which erroneous results can be achieved by failing to observe strict experiment design controls shifted burden of proof to applicants, and applicants failed to produce any evidence to overcome examiner's position.

There has also been a decision by the U.S. Court of Appeals Federal Circuit on an application involving "cold fusion".

See In re Swartz, 56 USPQ2d 1703 wherein it was held:

Claims in application that fails to meet utility requirement because invention is inoperative will also fail to meet enablement requirement because person skilled in art cannot practice invention, since application, in order to satisfy enablement of 35 U.S.C §112, must adequately disclose claimed invention so as to enable person skilled in art to practice invention at time of filing without undue experimentation, and since utility requirement of § 101 mandates that invention be operable to achieve useful results.

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U.S. Patent and Trademark Office properly rejected application claims directed to "cold fusion" process for lack of utility and enablement, since PTO provided substantial evidence that those skilled in art would reasonably doubt asserted utility of claimed invention, and found that applicant had not submitted evidence that concept of invention could have been practiced by person of ordinary skill without undue experimentation, and since applicant's conclusory allegations that PTO's decision on utility issue is not supported by substantial evidence, or that its conclusion of lack of enablement is incorrect as matter of law.

For a more recent decision by the U.S. Court of Appeals for the Federal Circuit on an application involving "cold fusion", see In re Dash et al, Decided: December 10, 2004, wherein it was stated:

As explained below, we construe the claims at issue to require the production of excess heat energy and to be directed to a method of achieving cold fusion. After that, we turn to the issues of utility and enablement, which here collapse into a single issue. See In re Brana, 51 F.3d 1550, 1564 (Fed. Cir. 1995) ("Obviously, if a claimed invention does not have utility, the specification cannot enable one to use it."). Given the scientific community's considerable doubt regarding the utility of "cold fusion" processes, we hold that the examiner established a *prima facie* case of lack of utility and enablement. Accordingly, the burden was shifted to Dash, and we hold that substantial evidence supports the Board's finding that Dash failed to meet that burden.

..... Dash's evidence that his invention achieved cold fusion likewise does not convince us that he rebutted the examiner's *prima facie* case of inoperability. Dash produced evidence regarding detection of tritium, transmutation of palladium, and physical transformation of the cathode, as well as corroborating experiments and calculations designed to show excess heat. For each type of evidence Dash produced, the examiner found at least one sound reason to disbelieve the evidence in either the literature that supported the *prima facie* case or in Dash's evidence itself. The Board affirmed the examiner's findings. The evidence cited by the examiner constitutes substantial evidence in support of the Board's decision. Accordingly, we hold that the Board acted reasonably in concluding that Dash did not make a showing sufficient to rebut the *prima facie* case of inoperability established by the examiner.

The Patent Office establishes a *prima facie* case of lack of utility by "showing that one of ordinary skill in the art would reasonably doubt the asserted utility [.]" Brana, 51 F.3d at 1566. Because the determination of whether an invention is operative is a question of fact, we review the Board's decision on this issue for

substantial evidence. Dash argues that the evidence that supported the examiner's prima facie case is invalid because it does not concern the invention as claimed and because the documents cited are anecdotal or not peer-reviewed. However, we are aware of no rule that forbids the examiner from relying on related technology, anecdotal information, or sources that are not peer-reviewed to establish a case of inoperability. These details merely go to the weight of the evidence, not whether it can be relied upon at all. Thus, we understand Dash's arguments as attacks on the weight the Board accorded to the cited information.

Substantial evidence supported the Board's finding that the examiner established a prima facie case of inoperability. While it may be ideal for the examiner to offer peer-reviewed data on precisely the claimed invention to establish such a case, such extreme certainty is not required. The examiner must only establish that a person of ordinary skill in the art would reasonably doubt the asserted utility. Brana, 51 F.3d at 1566. It was reasonable for the Board to conclude that the examiner has established such doubt based on the number and quality of cited references that debunked claims of cold fusion.

It is also noted that there has apparently been a court decision on cold fusion in Italy (e.g. see Italy-Cold Fusion & Judge's Verdict).

There is no adequate description or enabling disclosure of how and in what manner the energy input from the three fans is accounted for, or how and in what manner "the uncertainty in total power used by the fans and by the cell is less than 1mW" (specification page 6 line 1) hence the disclosure is insufficient and non-enabling.

There is no adequate description or enabling disclosure of how and in what manner voltage applied to the device within the calorimeter is measured at the thermal wall of the calorimeter as disclosed in, for example, the specification page 5 lines 22+, hence the disclosure is insufficient and non-enabling.

There is no adequate description or enabling disclosure of how and in what manner the pulse shape of the applied current is determined, hence the disclosure is insufficient and non-enabling.

There is no adequate description or enabling disclosure of how and in what manner the method to generate heat can be adapted for use in various applications as disclosed in, for example, page 8 second paragraph of the specification, hence the disclosure is insufficient and non-enabling.

There is no adequate description or enabling disclosure of what all materials are dissolved in the electrolyte as disclosed in, for example, page 9 line 2, hence the disclosure is insufficient and non-enabling.

There is no adequate description or enabling disclosure of how and in what manner persons skilled in the art are capable of determining what "similar chemicals" may be used to produce heat as disclosed in, for example page 9 second paragraph, hence the disclosure is insufficient and non-enabling.

Claim Rejections - 35 USC § 101

35 U.S.C. 101 reads as follows:

Whoever invents or discovers any new and useful process, machine, manufacture, or composition of matter, or any new and useful improvement thereof, may obtain a patent therefor, subject to the conditions and requirements of this title.

4. Claims 10, 13 and 17 are rejected under 35 U.S.C. 101 because the invention as disclosed is inoperative and therefore lacks utility.

The reasons that the invention as disclosed is operative are the same as the reasons set forth in section 3 above as to why the specification is objected to and the reasons set forth in said section 3 above are accordingly incorporated herein.

As set forth in section 3 above, there is no reputable evidence of record to indicate the invention has been reduced to the point of providing in current available form, an operative cold fusion system. The invention is not considered as meeting the requirements of 35 U.S.C. 101 as being "useful". Note in this respect, page 89 of Huizenga (I). Said page 89 reproduces the conclusion of the 1989 final report of the DOE/ERAB panel on cold fusion. Conclusion (I) states that there is no "convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion".

Now, even after 15 years of cold fusion research and according to one estimate by Review #14, an expenditure of more than 60 million dollars, the Dec. 1, 2004 final report of the DOE panel on cold fusion still has a similar conclusion.

Note the following quote from the Dec. 1, 2004 "Report of The Review of Low Energy Nuclear Reactions" under the heading "Conclusion":

"While significant progress has been made in the sophistication of calorimeters since the review of this subject in 1989, the conclusions reached by the reviewers today are similar to those found in the 1989 review."

Applicant at best, has set forth what may be considered a concept or an object of scientific research. However, it has been held that such does not present a utility within the meaning of 35 U.S.C. 101. See Brenner v. Manson, 148 U.S.P.Q. 689.

Additionally, it is well established that where as here, the utility of the claimed invention is based upon allegations that border on the incredible or allegations that would not be readily accepted by a substantial portion of the

scientific community (note the documents relied on by the examiner in section 3 above), sufficient substantiating evidence of operability must be submitted by applicant. Note In re Houghton, 167 USPQ 687 (CCPA 1970); In re Ferens, 163 USPQ 609 (CCPA 1969); Puharich v. Brenner, 162 UPSQ 136 (CADA 1969); In re Pottier, 152 USPQ 407 (CCPA 1967); In re Ruskin, 148 USPQ 221 (CCPA 1996); In re Citron, 139 USPQ 516 (CCPA 1963); and In re Novak, 134 USPQ 335 (CCPA 1962).

Requirement for information under 37 CFR 1.105.

5. Applicant's specification refers to experiments utilizing applicant's technique wherein excess heat was produced.

Indeed, due at least in part to the issues of non-reproducibility and non-predictability, it is common in the cold fusion art for many experimental runs (if not most), to show a negative result rather than a positive result (e.g. see Miles et al (II), Murray (II), Murray (III), page 77 of Hoffman).

Accordingly, applicant is required under 37 C.F.R. 1.105 to indicate the total number of experimental runs and to indicate how many runs (out of said total number of runs) produced measurable amounts of helium, excess heat and/or transmutation products. A statement by applicant that this information is unknown and/or not readily available will be accepted as a complete response to this requirement.

It appears that other artisans have attempted to replicate applicant's experiment and experimental results without obtaining reproducible results.

Accordingly, applicant is further required under 37 C.F.R. 1.105 to disclose the total number of experimental runs (indicating the number of runs that gave positive results) for all other artisans applicant is aware of having tried to replicate applicants experiment and/or experimental results. Again, a statement by applicant that this information is unknown and/or not readily available will be accepted as a complete response to this requirement.

In regard to the requirement for information under 37 C.F.R. 1.105, notice is taken of the January 3, 2005 Decision of the United States Court of Appeals for the Federal Circuit in the case of Star Fruits S.N.C. v. United States.

Pertinent portions of said Jan. 3, 2005 decision have been reproduced below.

Under 37 C.F.R. §1.105 the Office can require information that does not directly support a rejection. An agency's interpretation of its own regulations is entitled to substantial deference and will be accepted unless it is plainly erroneous or inconsistent with the regulation. See Eli Lilly & Co. v. Bd. Of Regents of the Univ. of Wa., 334 F. 3d 1264, 1266 (Fed. Cir. 2003). Here, the Office considered information concerning any sale or public distribution of the claimed invention and any information concerning Breeder's Rights applications or grants as within the authorized scope of a Requirement For Information under section 1.105.

This interpretation is not plainly erroneous or inconsistent with the regulation. Congress has delegated to the Office the rulemaking power to "establish regulations, not inconsistent with law, which-(A) shall govern the conduct of proceedings in the Office." 35 U.S.C. § 2(b)(2) (2000) (formerly at 35 U.S.C. § 6(a) (1988), see Merck & Co. v. Kessler, 80 F. 3d 1543, 1549-50 (Fed. Cir. 1996)); Stevens v. Tamai, 366 F.3d 1325, 1333 (Fed. Cir. 2004). Section 1.105 stems from an initiative entitled Changes to Implement the Patent Business Goals. Through notice and comment rulemaking the Office made explicit the inherent authority of Office employees to require information from an applicant. The goal is to "encourage" employees to use that power to "perform the best quality examination possible." 65 Fed. Reg. 54,604, at 54,633

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(September 8, 2000) (to be codified at 37 C.F.R. pts. 1, 3, 5, 10); see also 64 Fed. Reg. 53,772 (proposed October 4, 1999); 63 Fed. Reg. 53,498 (proposed October 5, 1998). The final rule permits that “the examiner or other Office employee may require the submission ... of such information as may be reasonably necessary to properly examiner or treat the matter.” 37 C.F.R. 1.105(a)(1) (emphasis added).

We think it clear that “such information as may be reasonably necessary to properly examiner or treat the matter,” 37 C.F.R. 1.105(a)(1), contemplates information relevant to examination either procedurally or substantively. It includes a zone of information beyond that defined by section 1.56 as material to patentability, and beyond that which is directly useful to support a rejection or conclusively decide the issue of patentability. Several observations militate in favor of this conclusion.

First, under the current regulations an applicant has an affirmative duty to disclose information material to patentability. See 37 C.F.R. § 1.56. Because an applicant already has a duty to disclose this information, it makes no sense for the Office to promulgate a rule empowering it to “require the submission” of information the applicant is required to submit in the first instance.

Second, section 1.105 identifies the required information as that information “reasonably necessary to property examiner or treat the matter” instead of that information “material to patentability.” Under ordinary principles of interpretation, the choice of different language indicates a different intended meaning.

Third, the plain language of the regulation contemplates requirements for information that go beyond information required by section 1.56. For example, “any non-patent literature ... by any of the inventors, that relates to the claimed invention[,]” 37 C.F.R. § 1.105(a)(1)(iii) (emphasis added), could include sales brochures, catalogues, or PBR applications or grants. “[A]ny use of the claimed invention known to any of the inventors at the time the application was filed notwithstanding the date of the use,” id. § 1.105(a)(1)(vii) (emphasis added), could refer to uses that would not affect patentability at all. Likewise, information directed to whether a search was conducted and what was searched, id § 1.105(a)(1)(ii), is not necessarily required by section 1.56. Other requirements for information are also foreseeable under the “reasonably necessary to property examiner or treat the matter” standard. For instance, it might be reasonably necessary for the Office to require an explanation of technical material in a publication, such as one of the inventor’s publications, or require the applicant’s comments on a recent Federal Circuit opinion and how that opinion affects examination. See, e.g., 65 Fed. Reg. at 54,634. Although this information improves the quality and efficiency of examination it is not necessarily information that an applicant is required to provide under section 1.56¹. In sum, we think that the Office’s interpretation of 37 C.F.R. § 1.105 conforms to the plain language of the regulation....

The Director is charged with the duty of deciding whether a patent should issue from an application. To perform that duty, the law must be applied to the facts at hand in any application. That the person charged with enforcement of the law, here an examiner, may sometimes disagree with the applicant on the theory or scope of the law to be applied is hardly surprising. So long as the request from the examiner for information is not arbitrary or capricious, the applicant cannot impede the examiner's performance of his duty by refusing to comply with an information requirement which proceeds from the examiner's view of the scope of the law to be applied to the application at hand. To allow such interference would have the effect of forcing the Office to make patentability determinations on insufficient facts and information. Such conduct inefficiently shifts the burden of obtaining information that the applicant is in the best position to most cheaply provide onto the shoulders of the Office and risks the systemic inefficiencies that attend the issue of invalid patents. Examination under such circumstances is neither fair and equitable to the public nor efficient.

Claim Rejections - 35 USC § 112

6. Claims 10, 13 and 17 are rejected under 35 U.S.C. 112, first paragraph, as failing to comply with the written description requirement. The claim(s) contains subject matter which was not described in the specification in such a way as to reasonably convey to one skilled in the relevant art that the inventor(s), at the time the application was filed, had possession of the claimed invention, for the reasons set forth in the objection to the specification, in section 3 above.

7. Claims 10, 13 and 17 are rejected under 35 U.S.C. 112, first paragraph, as failing to comply with the enablement requirement. The claim(s) contains subject matter which was not described in the specification in such a way as to enable one skilled in the art to which it pertains, or with which it is most nearly connected, to make and/or use the invention for the reasons set forth in the objection to the

specification, in section 3 above.

8. Claims 10, 13 and 17 are rejected under 35 U.S.C. 112, first paragraph, because the best mode contemplated by the inventors has not been disclosed.

The specification sets forth the positive results of cold fusion tests or experiments (e.g. see pages 4+ and Figures 2-6). Since said pages and figures set forth specific energy outputs detected for the tests or experiments, thus indicating that actual cold fusion cells or apparatuses were constructed and operated, the logical conclusion is that applicant was aware of all of the system parameters needed to give the indicated positive results but, failed to disclose such, said system parameters including the exact composition (including impurities and amounts thereof) of the electrolytic solution and of all components (such as the metal salts) therein; the composition size, dimensions of the electrodes and of the co-deposited layer (as well as the spacing there between); the requisite concentration of hydrogen and helium isotopes in the electrode; the applied currents, current densities and voltages; the instrument calibration prior to and during a run, test or experiment; the cleaning procedures (including methods/times/solutions/chemicals used, etc.) for each and every component of the cell; etc.

As indicated in MPEP 2165 and Union Carbide Corp. v. Borg-Warner, 193 USPQ 1:

"Failure to disclose the best mode need not rise to the level of active concealment or grossly inequitable conduct in order to support a rejection or

invalidate a patent. Where an inventor knows of a specific material that will make possible the successful reproduction of the effects claimed by the patent, but does not disclose it, speaking instead in terms of broad categories, the best mode requirement has not been satisfied".

See also, Spectra-Physics v. Coherent, 3 USPQ 2d 1737.

9. Claims 10, 13 and 17 are rejected under 35 U.S.C. 112, second paragraph, as being indefinite for failing to particularly point out and distinctly claim the subject matter which applicant regards as the invention.

- a. The claims are vague, indefinite and incomplete, particularly as to what all is meant by and is encompassed by the terms or phrases, "preparing", "excess heat producing material", "located", "is deposited", "applying an electric potential", "confirming the heat produced exceed the heat added", etc. as such have not been defined in either the specification or the claim, the metes and bounds of the claims are hence undefined.
- b. Claim 1 is vague, indefinite and incomplete in what all is meant by and encompassed by the phrase "excess heat producing material" because as discussed in section 3 above, those skilled in the art DO NOT KNOW what excess heat producing material is, hence the metes and bounds of the claim are undefined.
- c. Claim 1 is vague, indefinite and incomplete in what all is meant by and encompassed by the phrase "is deposited on said cathode surface" because the claim fails to disclose how and in what manner said deposition is accomplished, hence the metes and bounds of the claim are undefined.
- d. Claim 1, are vague, indefinite and incomplete in what all is meant by and

encompassed by the phrase "confirming that the heat so produced exceed the heat added" because as set forth in, for example section 3 above, those in the art do not know how and in what manner to produce excess heat, let alone "confirm" such alleged excess heat was even produced, hence the metes and bounds of the claims are undefined.

Claim Rejections - 35 USC § 102 and 35 USC § 103

The following is a quotation of the appropriate paragraphs of 35 U.S.C. 102 that form the basis for the rejections under this section made in this Office action:

A person shall be entitled to a patent unless —

(b) the invention was patented or described in a printed publication in this or a foreign country or in public use or on sale in this country, more than one year prior to the date of application for patent in the United States.

The following is a quotation of 35 U.S.C. 103(a) which forms the basis for all obviousness rejections set forth in this Office action:

(a) A patent may not be obtained though the invention is not identically disclosed or described as set forth in section 102 of this title, if the differences between the subject matter sought to be patented and the prior art are such that the subject matter as a whole would have been obvious at the time the invention was made to a person having ordinary skill in the art to which said subject matter pertains. Patentability shall not be negated by the manner in which the invention was made.

10. Claims 10, 13 and 17 are rejected under 35 U.S.C. 102(b) as being clearly anticipated by, or in the alternative rejected under 35 U.S.C. 103(a) as being unpatentable over applicants own publications listed on the attached PTO-892 as references X33 (Storms, How to produce the Pons-Fleischman Effect, 1996), V34

(Storms, A study of Electrolytic Tritium Production, 1990) and U34 (Storms, Ways to initiate a nuclear reaction in solid environments, 2001).

An Internet search of the instant Applicant's name reveals a plethora of published articles regarding Applicant's instant inventive concept. The instant Applicant is clearly well immersed within the cold fusion community, however a complete review of said published articles reveals that even applicant himself is STILL not aware of ALL the variables required to provide a working embodiment. Such awareness is critical to the understanding of the invention as already discussed in detail in the sections above.

For example, Applicant teaches preparing an electrolytic cell (see, for examples, all documents including any Figures), the use of a metal cathode (see, for examples, X33 page 3, section II, V34 ABSTRACT, U34 page 1 last paragraph, etc.), alleged heat producing materials deposited on the cathode surfaces (see, for examples, X33, page 3, first sentence of the second paragraph of section II., V34, Abstract, U34, page 1 last line, etc.), heavy water and LiOD dissolved therein (see, for examples, X33 page 6 second paragraph, second sentence, V34, Table VII, #127, etc,), applying a DC AND a PULSED electrical potential (wherein it is understood that the limitation "pulsed" reads on initially turning on and then turning off the power to the cell), and measuring energy added and allegedly created within said electrolytic cell. Applicant further teaches the use of platinum, palladium and gold as well as a myriad of other elements in the attempt to produce the alleged excess heat phenomenon, known

as "Cold Fusion" (see, for examples, X33 page 5, U34 ABSTRACT, second to last sentence, V34 ABSTRACT, etc.)

A review of these documents clearly set forth Applicant's theories including the variables those of ordinary skill in the art must vary/alter/experiment with in order to obtain an alleged working embodiment, however even applicant warns "Even now, all of the variables are not known." (See X33 page 2)

NOTE how Applicant specifically teaches "Apparently a special environment must first be created on or near the electrode surface before the rather novel nuclear reactions can start" (V34 Introduction) "These observations suggest that the active regions...are special regions that have grown or have been deposited on the surface of the cathode" (U34 page 1, last sentence) "The critical region is near the surface", and "the presence of certain impurities on the palladium surface are required...Such impurities can come from the anode, the electrolyte, or the container." (X33 pages 3 and 4) (Underlining added).

In reference X33 applicant further discusses the use of gold, lithium, platinum, palladium, etc.

If applicant is of the opinion that the references do not disclose applicants instant invention, then, at the time of the invention it would have been obvious to one of ordinary skill to utilize the methods and materials taught to be important and/or required within said references to arrive at the instant invention.

11. Claims 10, 13 and 17 are rejected under 35 U.S.C. 102(b) as being clearly anticipated by Pons (Pons et al. PCT/US90/01328).

Pons teaches applicant's instant inventive concept. For example, Pons teaches preparing an electrolytic cell (see, for examples, the entire document including the Figures), the use of a metal cathode (see, for examples, page 14, etc.), alleged heat producing materials deposited on the cathode surfaces (see, for examples, the paragraph spanning pages 6 and 7, section ID. "Thin Film Lattices" on Pages 31+, etc.), heavy water and LiOD dissolved therein (see, for examples, page 20, etc.,), applying a DC AND a PULSED electrical potential (see, for examples, page 22, etc.), and measuring energy added and allegedly created within said electrolytic cell (see, for examples, pages 36+, etc.). Pons further teaches the use of platinum, palladium and gold as well as a myriad of other elements in the attempt to produce the alleged excess heat phenomenon, known as "Cold Fusion" (see, for examples, pages 14+, etc.)

12. Claims 10, 13 and 17 are rejected under 35 U.S.C. 102(b) as being clearly anticipated by, or in the alternative rejected under 35 U.S.C. 103(a) as being unpatentable over Jones (Jones et al. PCT/US90/01328).

Jones teaches applicant's instant inventive concept. For example, Jones teaches preparing an electrolytic cell (see, for examples, the entire document including the claims and Figures), the use of a metal cathode (see, for examples, page 7, lines 25+, etc.), alleged heat producing materials deposited on the cathode

surfaces (see, for examples, page 9, page 14 lines 30+, page 15+, etc.), heavy water and LiOD dissolved therein (see, for examples, page 8 lines 31+, claims 9 and 10, etc.), applying a DC AND a PULSED electrical potential (see, for examples, page 10 lines 22, etc. Wherein it is understood that the limitation "pulsed" reads on initially turning on and then turning off the power to the cell), and measuring energy added and allegedly created within said electrolytic cell (see, for examples, pages 3+, etc.). Jones further teaches the use of platinum, palladium and gold as well as a myriad of other elements in the attempt to produce the alleged excess heat phenomenon, known as "Cold Fusion" (see, for examples, pages 7, lines 25-34, page 9 (materials deposited onto the surface) page 10 lines 1-13, etc.)

If applicant is of the opinion that the reference do not disclose applicants instant invention, then, at the time of the invention it would have been obvious to one of ordinary skill to utilize the methods and materials taught to be important and/or required within said references to arrive at the instant invention as such are no more than the use of commonly known materials and methods already known within the cold fusion art.

Conclusion

13. Examiner's Note: Examiner has cited particular columns and line numbers in the references as applied to the claims for the convenience of the applicant. Although the specified citations are representative of the teachings in the art and are applied to the specific limitations within the individual claim, other passages and figures may apply as

well. It is respectfully requested from the applicant, in preparing the responses, to fully consider ALL references in their entirety as potentially teaching all or part of the claimed invention, as well as the context of the passage as taught by the prior art or disclosed by the examiner.

14. Any inquiry concerning this communication or earlier communications from the examiner should be directed to Daniel L. Greene Jr. whose telephone number is (571) 272-6876. The examiner can normally be reached on Mon-Fri 8:30am - 5pm.

If attempts to reach the examiner by telephone are unsuccessful, the examiner's supervisor, Jack Keith can be reached on (571) 272-6878. The fax phone number for the organization where this application or proceeding is assigned is 571-273-8300.

15. Information regarding the status of an application may be obtained from the Patent Application Information Retrieval (PAIR) system. Status information for published applications may be obtained from either Private PAIR or Public PAIR. Status information for unpublished applications is available through Private PAIR only. For more information about the PAIR system, see <http://pair-direct.uspto.gov>. Should you have questions on access to the Private PAIR system, contact the Electronic Business Center (EBC) at 866-217-9197 (toll-free).

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